With regard to the possible resistance by surgeons to adopt these interventions, we think the availability of compelling evidence in support of these interventions to prevent lung injury will eliminate any practical concerns.

Dr Macedo and colleagues refer to a trial that will assess the effect of lung perfusion/ventilation during CPB. We look forward to their results, which will hopefully add further knowledge to face the continued ch allenge of lung injury during cardiac surgery.

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PULMONARY AUTOGRAFT FOR MITRAL VALVE REPLACEMENT: MAKING A SIMPLE PROCEDURE COMPLEX?

To the Editor:

I read with interest the article by Kumar and colleagues[1] about the use of pulmonary autografts for mitral valve replacement. I agree with the authors that cardiac surgeons working in Third World countries are faced with a major problem in the management of large numbers of patients with valvular disease. Most of those patients are of low socioeconomic and educational class, living in remote villages and mountains. There is no family practice or general practitioner system to provide early diagnosis and referral, postoperative follow-up, and anticoagulation control. Patients usually present late with advanced disease that makes repair impossible. The magnitude of the problem is escalated in children and women of childbearing age because we have no ideal valve substitute.

The pulmonary autograft concept is attractive but still has its drawbacks and complications. The procedure is complex and technically demanding, requiring at least double the usual crossclamp and bypass times. The procedure is not free of charge because there is a price for the homografts or preparation of the autografts, plus the added cost of complications. The mortality of approximately 15% reported by the authors is still high compared with single mitral valve replacement. Kabbani and colleagues[2] reported 5% early mortality and 6% late mortality, but as they stated, it is ''clearly related to the procedure,'' which entails higher overall mortality. The issue of converting a single-valve into a double-valve disease is irritating because we have to expect long-term sequelae of pulmonary and mitral valve failures. The main concern with pulmonary autografts or homografts is early calcification. The removal of calcified grafts, as experienced in aortic homografts, is usually disastrous and has a high complication rate, including left ventricular rupture. The same complications are associated with the use of aortic or mitral homografts for mitral valve replacement. All of these autografts and homografts have to compete with the standard stented bioprostheses, which are improving and technically easier to insert, and require no anticoagulation. At the present time, bioprosthetic valves remain the standard in these patients, and manufacturers have shown us better preservation and longer durability.[3]

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Reply to the Editor:

We are thankful to Dr Khaled E. Al-Ebrahim for his comments and critique. These were the same arguments raised when the Ross procedure for aortic valve replacement was first reported by Donald Ross in 1967.

The patients undergoing operation are young and clearly unsuitable for tissue valves. The surgery is indeed complex but provides superior hemodynamic results with a higher valve area. In addition, these patients only require an echocardiogram at yearly intervals, receive no medication 1 year after surgery, and do not require blood tests. The most important advantage is that it is a living valve, and we now have more than a 15-year follow-up of our own results for the Ross (aortic valve replacement) procedure. There has been no reoperation in the adults with a pulmonary homograft.

We believe these early results will translate into superior outcomes in